being the ſole object of my inquiries (having no favourite theory to defend) it brings no diſappointment along with it, under whatever unexpected ſhape it may appear. I hope that further Experiments may lead to the diſcovery of the cauſe why there is ſo little difference in the conducting powers of air of ſuch very different degrees of rarity, while there is ſo great a difference in the conducting powers of air, and of the Torricellian vacuum. At preſent, I ſhall not venture any conjectures upon the ſubject; but in the mean time I dare to aſſert, that the Experiments I have made may be depended on.

The time of my ſtay at Manheim being expired (having had the honour to attend thither his moſt Serene Highneſs the Elector Palatine, reigning Duke of Bavaria, in his late journey), I was prevented from purſuing theſe inquiries further at that time; but I ſhall not fail to recommence them the firſt leiſure moment I can find, which I fancy will be about the beginning of the month of November. In the mean time, to enable myſelf to purſue them with effect, I am ſparing neither labour nor expence to provide a complete apparatus neceſſary for my purpoſe; and his Electoral Highneſs has been graciouſly pleaſed to order M. ARTARIA (who is in his ſervice) to come to Munich to aſſiſt me. With ſuch a Patron as his moſt Serene Highneſs, and with ſuch an aſſiſtant as ARTARIA, I ſhall go on in my purſuits with chearfulneſs. Would to God that my labours might be as uſeful to others as they will be pleaſant to me.

I ſhall conclude this chapter with a ſhort account of ſome Experiments I have made to determine the conducting powers of water and of mercury; and with a table, ſhowing at one view the conducting powers of all the different mediums which I have examined.

Having filled the glaſs globe incloſing the bulb of the thermometer N° 4, firſt with water, and then with mercury, I made the following Experiments, to aſcertain the conducting powers of thoſe two Fluids.

| (Exp. N° 23.)<br>*Thermometer* N° 4.<br>Surrounded by *water.*<br>*Taken out of freezing water, and plunged into boiling water.* | | (Exp. N° 24, 25, and 26.)<br>*Thermometer* N° 4.<br>Surrounded by *mercury.*<br>*Taken out of freezing water, and plunged into boiling water.* | | | |
|---|---|---|---|---|---|
| Time elapſed. | Heat acquired. | Time elapſed. | | | Heat acquired. |
| | | Ex. N° 24. | Ex. N° 25. | Ex. N° 26. | |
| | 0° | | | | 0° |
| M. S. | 0 | M. S. | M. S. | M. S. | |
| 0 19 | 10 | 0 5 | 0 5 | 0 5 | 10 |
| 0 8 | 20 | 0 4 | 0 2 | 0 5 | 20 |
| 0 9 | 30 | 0 2 | 0 2 | 0 4 | 30 |
| 0 11 | 40 | 0 4 | 0 5 | 0 5 | 40 |
| 0 15 | 50 | 0 4 | 0 4 | 0 7 | 50 |
| 0 21 | 60 | 0 7 | 0 4 | 0 8 | 60 |
| 0 34 | 70 | 0 15 | 0 9 | 0 14 | 70 |
| 2 13 | 80 | Not obſerved. | 0 58 | Not obſerved. | 80 |
| 1 57 = total time of heating from 0° to 70°. | | 0 41 | 0 31 | 0 48 = total times of heating from 0° to 70°. | |

The total times of heating from 0° to 70° in the three Experiments with mercury being 41 ſeconds, 31 ſeconds, and 48 ſeconds, the mean of theſe times is $36\frac{2}{3}$ ſeconds; and as in the Experiment with water the time employed in acquiring the ſame degree of Heat was 1′ 57″ = 117 ſeconds, it appears from theſe Experiments, that the conducting power of mercury to that of water, under the circumſtances deſcribed, is as $36\frac{2}{3}$ to 117 inverſely, or as 1000 to 313. And hence it is plain, why mercury *appears* ſo much hotter, and ſo much colder, to the touch than water, when in fact it is of the ſame temperature: for the force or violence of the ſenſation of what appears *hot* or *cold* depends not entirely upon the temperature of the body exciting in us thoſe ſenſations, or upon the degree of Heat it actually poſſeſſes, but upon the *quantity* of Heat it is capable of communicating to us, or receiving from us, in any given ſhort period of time, or as the intenſity of the communication; and this depends in a great meaſure upon the conducting powers of the bodies in queſtion.

The ſenſation excited in us when we touch any thing that appears to us to be *hot* is the entrance of Heat into our bodies; that of *cold* is its exit; and whatever contributes to facilitate or accelerate this communication adds to the violence of the ſenſation. And this is another proof that the thermometer cannot be a juſt meaſure of the intenſity of the *ſenſible* Heat, or Cold, exiſting in bodies; or rather, that the touch does not afford us a juſt indication of their *real* temperatures.

## *A* TABLE *of the* CONDUCTING POWERS *of the under-mentioned* MEDIUMS *as determined by the fore-going Experiments.*

| Therm. N° 1. | Thermometer N° 4. | | | | | | |
|---|---|---|---|---|---|---|---|
| *Taken out of freezing water, and plunged into boiling water.* | | | | | | | |
| Time elapſed. | | | | | | | |
| Torricellian Vacuum (Exp. N° 3, 4. and 13.) | Common air, denſity = 1, (Exp. N° 20.) | Rarefied air, denſity = 1/4 (Exp. N° 21.) | Rarefied air, denſity = 1/24 (Exp. N° 22.) | Moiſt air (Exp. N° 18.) | Water (Exp. N° 23.) | Mercury (Exp. N° 24, 25, and 26.) | Heat acquired. |
| | | | | | | | 0° |
| M. S. | M. S. | M. S. | M. S. | M. S. | M. S. | M. S. | 0 |
| 0 52 | 0 31 | 0 31 | 0 29 | 0 6 | 0 19 | 0 5 | 10 |
| 0 58 | 0 40 | 0 38 | 0 36 | 0 4 | 0 8 | 0 3⅔ | 20 |
| 1 3 | 0 41 | 0 44 | 0 49 | 0 5 | 0 9 | 0 2⅔ | 30 |
| 1 18 | 0 47 | 0 51 | 1 1 | 0 9 | 0 11 | 0 4⅔ | 40 |
| 1 25 | 1 4 | 1 7 | 1 1 | 0 18 | 0 15 | 0 5 | 50 |
| 1 58 | 1 25 | 1 19 | 1 24 | 0 26 | 0 21 | 0 6⅓ | 60 |
| 3 19 | 2 28 | 2 27 | 2 31 | 0 43 | 0 34 | 0 12⅔ | 70 |
| 11 57 | 10 17 | 10 21 | —— | 7 45 | 2 13 | 0 58 | 80 |
| 10 53 | 7 36 | 7 37 | 7 51 | 1 51 | 1 57 | 0 36⅔ = total | |

times of heating from 0° to 70°.

In determining the relative conducting powers of theſe mediums, I have compared the times of the heating of the thermometers from 0° to 70° inſtead of taking the whole times from 0° to 80°, and this I have done on account of the ſmall variation in the Heat of the boiling water ariſing from the variation of the weight of the atmoſphere, and alſo on account of the very ſlow motion of the mercury between the 70th and the 80th degrees, and the difficulty of determining the preciſe moment when the mercury arrives at the 80th degree.

Taking now the conducting power of mercury $=1000$, the conducting powers of the other mediums, as determined by theſe Experiments, will be as follows, *viz.*

| | |
|---|---|
| Mercury - - - | 1000 |
| Moiſt air - - - | 330 |
| Water - - - | 313 |
| Common air, denſity $=1$ | $80\frac{41}{100}$ |
| Rarefied air, denſity $=\frac{1}{4}$ | $80\frac{23}{100}$ |
| Rarefied air, denſity $=\frac{1}{24}$ | 78 |
| The Torricellian vacuum . | 55 |

And in theſe proportions are the quantities of Heat which theſe different mediums are capable of tranſmitting in any given time; and conſequently theſe numbers expreſs the relative *ſenſible* temperatures of the mediums, as well as their conducting powers. How far theſe deciſions will hold good under a variation of circumſtances experiment only can determine. This is certainly a ſubject of inveſtigation not leſs curious in itſelf than it is intereſting to mankind; and I wiſh that what I have done may induce others to turn their attention to this long neglected field of experimental inquiry. For my own part, I am determined not to quit it.

In the future proſecution of theſe inquiries, I do not mean to confine myſelf ſolely to the determining of, the conducting powers of Fluids; on the contrary, ſolids, and particularly ſuch bodies as are made uſe of for cloathing, will be principal ſubjects of my future Experiments. I have indeed already begun theſe reſearches, and have made ſome progreſs in them; but I forbear to anticipate a matter which will be the ſubject of a future communication.

# CHAP. II.

*The relative Warmth of various Subſtances uſed in making artificial Cloathing, determined by Experiment.—Relative Warmth of Coverings of the ſame Thickneſs, and formed of the ſame Subſtance, but of different Denſities.—Relative Warmth of Coverings formed of equal Quantities of the ſame Subſtance, diſpoſed in different Ways.—Experiments made with a View to determining how far the Power which certain Bodies poſſeſs of confining Heat depends on their chemical Properties.—Experiments with* Charcoal—*with* Lampblack—*with* Woodaſhes—*Striking Experiments with* Semen Lycopodii.—*All theſe Experiments indicate that the Air which occupies the Interſtices of Subſtances uſed in forming Coverings for confining Heat, acts a very important Part in that Operation.—Thoſe Subſtances appear to prevent the Air from conducting the Heat.—An Inquiry concerning the Manner in which this is effected.—This Inquiry leads to a deciſive Experiment from the Reſult of which it appears that* Air is a perfect Non-conductor of Heat.—*This Diſcovery affords the Means of explaining a Variety of intereſting Phenomena in the Œconomy of Nature.*

[Read before the Royal Society, January 19, 1792.]

THE confining and directing of Heat are objects of ſuch vaſt importance in the œconomy of human life, that I have been induced to confine my reſearches chiefly to thoſe points, conceiving that very great advantages to mankind could not fail to be derived from the diſcovery of any new facts relative to theſe operations.

If the laws of the communication of Heat from one body to another were known, meaſures might be taken with certainty, in all caſes, for confining it, and directing its operations, and this would not only be productive of great œconomy in the articles of fuel and clothing, but would likewiſe greatly increaſe the comforts and conveniencies of life; objects of which the philoſopher ſhould never loſe ſight.

The route which I have followed in this inquiry is that which I thought bid faireſt to lead to uſeful diſcoveries. Without embarraſſing myſelf with any particular theory, I have formed to myſelf a plan of experimental inveſtigation, which I conceived would conduct me to the knowledge of *certain facts*, of which we are now ignorant, or very imperfectly informed, and with which it is of conſequence that we ſhould be made acquainted.

The firſt great object which I had in view in this enquiry was to aſcertain, if poſſible, the cauſe of the warmth of certain bodies; or the circumſtances upon which their power of confining Heat depends. This, in other words, is no other than to determine the cauſe of the conducting and non-conducting power of bodies, with regard to Heat.

To this end I began by determining by actual experiment the relative conducting powers of various bodies of very different natures, both fluids and ſolids, of ſome of which Experiments I have already given an account in the Paper above mentioned, which is publiſhed in the Tranſactions of the Royal Society for the year 1786; I ſhall now, taking up the matter where I left it, give the continuation of the hiſtory of my reſearches.

Having diſcovered that the Torricellian vacuum is a much worſe conductor of Heat than common air, and having aſcertained the relative conducting powers of air, of water, and of mercury, under different circumſtances, I proceeded to examine the conducting powers of various *ſolid bodies*, and particularly of ſuch ſubſtances as are commonly made uſe of for clothing.

The method of making theſe Experiments was as follows: a mercurial thermometer, (ſee Fig. 4,) whoſe bulb was about $\frac{55}{100}$ of an inch in diameter, and its tube, about 10 inches in length, was ſuſpended in the axis of a cylindrical glaſs tube, about $\frac{3}{4}$ of an inch in diameter, ending with a globe $1\frac{6}{10}$ inch in diameter, in ſuch a manner that the centre of the bulb of the thermometer occupied the centre of the globe; and the ſpace between the internal ſurface of the globe and the ſurface of the bulb of the thermometer being filled with the ſubſtance whoſe conducting power was to be determined, the inſtrument was heated in boiling water, and afterwards being plunged into a freezing mixture of pounded ice and water, the times of cooling were obſerved, and noted down.

The tube of the thermometer was divided at every tenth degree from 0°, or the point of freezing, to 80°, that of boiling water, and theſe diviſions being marked upon the tube with the point of a diamond, and the cylindrical tube being left empty, the height of the mercury in the tube of the thermometer was ſeen through it.

The thermometer was confined in its place by means of a ſtopple of cork, about 1½ inch long, fitted to the mouth of the cylindrical tube, through the centre of which ſtopple the end of the tube of the thermometer paſſed, and in which it was cemented.

The operation of introducing into the globe the ſubſtances whoſe conducting powers are to be determined, is performed in the following manner; the thermometer being taken out of the cylindrical tube, about two-thirds of the ſubſtance which is to be the ſubject of the Experiment are introduced into the globe; after which, the bulb of the thermometer is introduced a few inches into the cylinder; and, after it, the remainder of the ſubſtance being placed round about the tube of the thermometer; and laſtly, the thermometer being introduced farther into the tube, and being brought into its proper place, that part of the ſubſtance which, being introduced laſt, remains in the cylindrical tube above the bulb of the thermometer, is puſhed down into the globe, and placed equally round the bulb of the thermometer by means of a braſs wire which is paſſed through holes made for that purpoſe in the ſtopple cloſing the end of the cylindrical tube.

As this inſtrument is calculated merely for meaſuring the paſſage of Heat in the ſubſtance whoſe conducting power is examined, I ſhall give it the name of *paſſage-thermometer*, and I ſhall apply the ſame appellation to all other inſtruments conſtructed upon the ſame principles, and for the ſame uſe, which I may in future have occaſion to mention; and as this inſtrument has been ſo particularly deſcribed, both here, and in my former Paper upon the ſubject of Heat, in ſpeaking of any others of the ſame kind in future it will not be neceſſary to enter into ſuch minute details. I ſhall, therefore, only mention their *ſizes*, or the diameters of their bulbs, the diameters of their globes, the diameters of their cylinders, and the lengths and diviſions of their tubes, taking it for granted that this will be quite ſufficient to give a clear idea of the inſtrument.

In moſt of my former Experiments, in order to aſcertain the conducting power of any body, the body being introduced into the globe of the paſſage-thermometer, the inſtrument was cooled to the temperature of freezing water, after which, being taken out of the ice water, it was plunged ſuddenly into boiling water, and the times of heating from ten to ten degrees were obſerved and noted; and I ſaid that theſe times were as the conducting power of the body inverſely; but in the Experiments of which I am now about to give an account, I have in general reverſed the operation; that is to ſay, inſtead of obſerving the times of heating, I have firſt heated the body in boiling water, and then plunging it into a mixture of pounded ice and ice-cold water, I have noted the times taken up in cooling.

I have preferred this laſt method to the former, not only on account of the greater eaſe and convenience with which a thermometer, plunged into a mixture of water, may be obſerved, than when placed in a veſſel of boiling water, and ſurrounded by hot ſteam, but alſo on account of the greater accuracy of the Experiment, the heat of boiling water varying with the variations of the preſſure of the atmoſphere: conſequently the Experiments made upon different days will have different reſults, and of courſe, ſtrictly ſpeaking, cannot be compared together; but the temperature of pounded ice and water is ever the ſame, and of courſe the reſults of the Experiments are uniform.

In heating the thermometer, I did not in general bring it to the temperature of the boiling water, as this temperature, as I have juſt obſerved, is variable; but when the mercury had attained the 75° of its ſcale, I immediately took it out of the boiling water, and plunged it into the ice and water; or, which I take to be ſtill more accurate, ſuffering the mercury to riſe a degree or two above 75°, and then taking it out of the boiling water, I held it over the veſſel containing the pounded ice and water, ready to plunge it into that mixture the moment the mercury, deſcending, paſſes the 75°.

Having a watch at my ear which beat half ſeconds (which I counted), I noted the time of the paſſage of the mercury over the diviſions of the thermometer, marking 70° and every tenth degree from it, deſcending to 10° of the ſcale. I continued the cooling to 0°, or the temperature of the ice and water, in very few inſtances, as this took up much time, and was attended with no particular advantage, the determination of the times taken up in cooling 60 degrees of Reaumur's ſcale, that is to ſay, from 70° to 10°, being quite ſufficient to aſcertain the conducting power of any body whatever.

During the time of cooling in ice and water, the thermometer was conſtantly moved about in this mixture from one place to another; and there was always ſo much pounded ice mixed with the water, that the ice appeared above the ſurface of the water; the veſſel, which was a large earthen jar, being firſt quite filled with pounded ice, and the water being afterwards poured upon it, and freſh quantities of pounded ice being added as the occaſion required.

Having deſcribed the apparatus made uſe of in theſe Experiments, and the manner of performing the different operations, I ſhall now proceed to give an account of the Experiments themſelves.

My firſt attempt was to diſcover the relative conducting powers of ſuch ſubſtances as are commonly made uſe for clothing; accordingly, having procured a quantity of *raw ſilk*, as ſpun by the worm, *ſheep's wool*, *cotton wool*, *linen* in the form of the fineſt lint, being the ſcrapings of very fine Iriſh linen, the fineſt part of the *fur of the beaver* ſeparated from the ſkin, and from the long hair, the fineſt part of the *fur of a white Ruſſian hare*, and

*Eider down ;* I introduced ſucceſſively 16 grains in weight of each of theſe ſubſtances into the globe of the paſſage-thermometer, and placing it carefully and equally round the bulb of the thermometer, I heated the thermometer in boiling water, as before deſcribed, and taking it out of the boiling water, plunged it into pounded ice and water, and obſerved the times of cooling.

But as the interſtices of theſe bodies thus placed in the globe were filled with air, I firſt made the Experiment with air alone, and took the reſult of that Experiment, as a ſtandard by which to compare all the others; the reſults of three Experiments with air were as follows :

The bulb of the Thermometer ſurrounded by air.

| Heat loſt. | Exp. No. 1. Time elapſed. | Exp. No. 2. Time elapſed. | Heat acquired. | Exp. No. 3. Time elapſed. |
|---|---|---|---|---|
| 70° | — | — | 10° | — |
| 60° | 38″ | 38″ | 20° | 39″ |
| 50° | 46 | 46 | 30° | 43 |
| 40° | 59 | 59 | 40° | 53 |
| 30° | 80 | 79 | 50° | 67 |
| 20° | 122 | 122 | 60° | 96 |
| 10° | 231 | 230 | 70° | 175 |
| Total times. | 576 | 574 | — | 473 |

The following Table ſhows the reſults of the Experiments, with the various ſubſtances therein mentioned:

| Heat loſt. | Air. | Raw ſilk, 16 grs. | Sheep's wool, 16 grs. | Cotton wool, 16 grs. | Fine lint, 16 grs. | Beavers fur, 16 grs. | Hares fur, 16 grs. | Eider down, 16 grs. |
|---|---|---|---|---|---|---|---|---|
| | Exp. 1. | Exp. 4. | Exp. 5. | Exp. 6. | Exp. 7. | Exp. 8. | Exp. 9. | Ex. 10. |
| 70° | — | — | — | — | — | — | — | — |
| 60° | 38″ | 94″ | 79″ | 83″ | 80″ | 99″ | 97″ | 98″ |
| 50° | 45 | 110 | 95 | 95 | 93 | 116 | 117 | 116 |
| 40° | 59 | 133 | 118 | 117 | 115 | 153 | 144 | 146 |
| 30° | 80 | 185 | 162 | 152 | 150 | 185 | 193 | 192 |
| 20° | 122 | 273 | 258 | 221 | 218 | 265 | 270 | 268 |
| 10° | 231 | 489 | 426 | 378 | 376 | 478 | 494 | 485 |
| Total times. | 576 | 1284 | 1118 | 1046 | 1032 | 1296 | 1315 | 1305 |

Now the *warmth* of a body, or its power to confine Heat, being as its power of reſiſting the paſſage of Heat through it, (which I ſhall call its *non-conducting power*,) and the time taken up by any body in cooling, which is ſurrounded by any medium through which the Heat is obliged to paſs, being, *cæteris paribus*, as the reſiſtance which the medium oppoſes to the paſſage of the Heat, it appears that the *warmth* of the bodies mentioned in the foregoing Table are as the times of cooling; the *conducting powers* being inverſely as thoſe times, as I have formerly ſhown.

From the reſults of the foregoing Experiments it appears, that of the ſeven different ſubſtances made uſe of, hares fur and Eider down were the warmeſt; after theſe came beavers fur; raw ſilk; ſheep's wool; cotton wool; and laſtly, lint, or the ſcrapings of fine linen; but I acknowledge that the differences in the warmth of theſe ſubſtances were much leſs than I expected to have found them.

Suſpecting that this might ariſe from the volumes or ſolid contents of the ſubſtances being different, (though their weights were the ſame,) ariſing from the difference of their ſpecific gravities; and as it was not eaſy to determine the ſpecific gravities of theſe ſubſtances with accuracy, in order to ſee how far any known difference in the volume or quantity of the ſame ſubſtance, confined always in the ſame ſpace, would add to or diminiſh the time of cooling, or the apparent warmth of the covering, I made the three following Experiments.

In the firſt, the bulb of the thermometer was ſurrounded by 16 grains of Eider down; in the ſecond by 32 grains; and in the third by 64 grains; and in all theſe Experiments the ſubſtance was made to occupy exactly the ſame ſpace, viz. the whole internal capacity of the glaſs globe, in the centre of which the bulb of the thermometer was placed; conſequently the thickneſs of the covering of the thermometer remained the ſame, while its denſity was varied in proportion to the numbers 1, 2, and 4.

The reſults of theſe Experiments were as follow:

The bulb of the thermometer being ſurrounded by Eider down.

| Heat loſt. | 16 grains. | 32 grains. | 64 grains. |
|---|---|---|---|
| | (Exp. No. 11.) | (Exp. No. 12.) | (Exp. No. 13.) |
| 70° | — | — | — |
| 60° | 97″ | 111″ | 112″ |
| 50° | 117 | 128 | 130 |
| 40° | 145 | 157 | 165 |
| 30° | 192 | 207 | 224 |
| 20° | 267 | 304 | 326 |
| 10° | 486 | 565 | 658 |
| Total times. | 1304 | 1472 | 1615 |

Without ſtopping at preſent to draw any particular concluſions from the reſults of theſe Experiments, I ſhall proceed to give an account of ſome others, which will afford us a little further inſight into the nature of ſome of the circumſtances upon which the warmth of covering depends.

Finding, by the laſt Experiments, that the denſity of the covering added ſo conſiderably to the warmth of it, its thickneſs remaining the ſame, I was now deſirous of diſcovering how far the internal ſtructure of it contributed to render it more or leſs pervious to Heat, its thickneſs and quantity of matter remaining the ſame. By internal ſtructure, I mean the diſpoſition of the parts of the ſubſtance which forms the covering; thus they may be extremely divided, or very fine, as raw ſilk as ſpun by the worms, and they may be equally diſtributed through the whole ſpace they occupy; or they may be coarſer, or in larger maſſes, with larger interſtices, as the ravelings of cloth, or cuttings of threads.

If Heat paſſed *through* the ſubſtances made uſe of for covering, and if the warmth of the covering depended ſolely upon the difficulty which the Heat meets with in its paſſage through the ſubſtances, *or ſolid parts*, of which they are compoſed; in that caſe, the warmth of covering would be always, *cæteris paribus*, as the quantity of materials of of which it is compoſed; but that this is not the caſe, the following, as well as the foregoing Experiments clearly evince.

Having, in the Experiment N° 4, aſcertained the warmth of 16 grains of raw ſilk, I now repeated the Experiment with the ſame quantity, or weight, of the ravelings of white taffety, and afterwards with a like quantity of common ſewing ſilk, cut into lengths of about two inches.

The following Table ſhows the reſults of theſe three Experiments:

| Heat loſt. | Raw ſilk, 16 grs. | Ravelins of taffety, 16 grs. | Sewing ſilk cut into lenghts, 16 grs. |
|---|---|---|---|
| | Exp. 4. | Exp. 14. | Exp. 15. |
| 70° | — | — | — |
| 60° | 94″ | 90″ | 67″ |
| 50° | 110 | 106 | 79 |
| 40° | 133 | 128 | 99 |
| 30° | 185 | 172 | 135 |
| 20° | 273 | 246 | 195 |
| 10° | 489 | 427 | 342 |
| Total times. | 1284 | 1169 | 917 |

Here, notwithſtanding that the quantities of the ſilk were the ſame in the three Experiments, and though in each of them it was made to occupy the ſame ſpace, yet the warmth of the coverings which were formed were very different, owing to the different diſpoſition of the material.

The raw ſilk was very fine, and was very equally diſtributed through the ſpace it occupied, and it formed a warm covering.

The ravelings of taffety were alſo fine, but not ſo fine as the raw ſilk, and of courſe the interſtices between its threads were greater, and it was leſs warm; but the cuttings of ſewing ſilk were very coarſe, and conſequently it was very unequally diſtributed in the ſpace in which it was confined; and it made a very bad covering for confining Heat.

It is clear from the reſults of the five laſt Experiments, that the air which occupies the interſtices of bodies, made uſe of for covering, acts a very important part in the operation of confining Heat; yet I ſhall poſtpone the examination of that circumſtance till I ſhall have given an account of ſeveral other Experiments, which, I think, will throw ſtill more light upon that ſubject.

But, before I go any further, I will give an account of three Experiments which I made, or rather the ſame Experiment which I repeated three times the ſame day, in order to ſee how far they may be depended on, as being regular in their reſults.

The glaſs globe of the paſſage-thermometer being filled with 16 grains of cotton-wool, the inſtrument was heated and cooled three times ſucceſſively, when the times of cooling were obſerved as follows:

| Heat loſt. | Exp. 16. | Exp. 17. | Exp. 18. |
|---|---|---|---|
| 70° | — | — | — |
| 60° | 82″ | 84″ | 83″ |
| 50° | 96 | 95 | 95 |
| 40° | 118 | 117 | 116 |
| 30° | 152 | 153 | 151 |
| 20° | 221 | 221 | 220 |
| 10° | 380 | 377 | 377 |
| Total times. | 1049 | 1047 | 1042 |

The difference of the times of cooling in theſe three Experiments were extremely ſmall; but regular as theſe Experiments appear to have been in their reſults, they were not more ſo than the other Experiments made in the ſame way, many of which were repeated two or three times, though, for the ſake of brevity, I have put them down as ſingle Experiments.

But to proceed in the account of my inveſtigations relative to the cauſes of the warmth of warm clothing. Having found that the fineneſs and equal diſtribution of a body or ſubſtance made uſe of to form a covering to confine Heat, contributes ſo much to the warmth of the covering, I was deſirous, in the next place to ſee the effect of condenſing the covering, its quantity of matter remaining the ſame, but its thickneſs being diminiſhed in proportion to the increaſe of its denſity.

The Experiment I made for this purpoſe was as follows:—I took 16 grains of common ſewing ſilk, neither very fine nor very coarſe, and winding it about the bulb of the thermometer in ſuch a manner that it entirely covered it, and was as nearly as poſſible of the ſame thickneſs in every part, I replaced the thermometer in its cylinder and globe, and heating it in boiling water, cooled it in ice and water, as in the foregoing Experiments. The reſults of the Experiment were as may be ſeen in the following Table; and in order that it may be compared with thoſe made with the ſame quantity of ſilk differently diſpoſed of, I have placed thoſe Experiments by the ſide of it:

| Heat left. | Raw ſilk, 16 grs. | Fine ravelings of taffety, 16 grs. | Sewing ſilk cut into lengths, 16 grs. | Sewing ſilk, 16 grs. wound round the bulb of the thermometer. |
|---|---|---|---|---|
| | Exp. No. 4. | Exp. No. 14. | Exp. No. 15. | Exp. No. 19. |
| 70° | — | — | — | — |
| 60° | 94″ | 90″ | 67″. | 46″ |
| 50° | 110 | 106 | 79 | 62 |
| 40° | 133 | 128 | 99 | 85 |
| 30° | 185 | 172 | 135 | 121 |
| 20° | 273 | 246 | 195 | 191 |
| 10° | 489 | 427 | 342 | 399 |
| Total times. | 1214 | 1169 | 917 | 904 |

It is not a little remarkable, that, though the covering formed of ſewing ſilk wound round the bulb of the thermometer in the 19th Experiment, appeared to have ſo little power of confining the Heat when the inſtrument was very hot, or when it was firſt plunged into the ice and water, yet afterwards, when the Heat of the thermometer approached much nearer to that of the ſurrounding medium, its power of confining the Heat which remained in the bulb of the thermometer appeared to be even greater than that of the ſilk in the Experiment N° 15, the time of cooling from 20° to 10° being in the one 399″, and in the other 342″. The ſame appearance was obſerved in the following Experiments, in which the bulb of the thermometer was ſurrounded by threads of *wool*, of *cotton*, and of *linen*, or *flax*, wound round it, in the like manner as the ſewing ſilk was wound round it in the laſt Experiment.

The following Table ſhows the reſults of theſe Experiments, with the threads of various kinds; and that they may the more eaſily be compared with thoſe made with the ſame quantity of the ſame ſubſtances in a different form, I have placed the accounts of theſe Experiments by the ſide of each other. I have alſo added the account of an Experiment, in which 16 grains of fine linen cloth were wrapped round the bulb of the thermometer, going roung it nine times, and being bound together at the top and bottom of it, ſo as completely to cover it.

| Heat loſt. | *Sheep's wool*, 16 grains, ſurrounding the bulb of the thermometer. | *Woollen thread*, 16 grains, wound round the bulb of the thermometer. | *Cotton wool*, 16 grains, ſurrounding the bulb of the thermometer. | *Cotton thread*, 16 grains, wound round the bulb of the thermometer. | *Lint*, 16 grains, ſurrounding the bulb of the thermometer. | *Linen thread*, 16 grains, wound round the bulb of the thermometer. | *Linen cloth*, 16 grains, wrapped round the bulb of the thermometer. |
|---|---|---|---|---|---|---|---|
| | Exp. 5. | Ex. 20. | Exp. 6. | Ex. 21. | Exp. 7. | Ex. 22. | Exp. 23. |
| 70° | — | — | — | — | — | — | — |
| 60° | 79″ | 46″ | 83″ | 45″ | 80″ | 46″ | 42″ |
| 50° | 95 | 63 | 95 | 60 | 93 | 62 | 56 |
| 40° | 118 | 89 | 117 | 83 | 115 | 83 | 74 |
| 30° | 162 | 126 | 152 | 115 | 150 | 117 | 108 |
| 20° | 238 | 200 | 221 | 179 | 218 | 180 | 168 |
| 10° | 426 | 410 | 378 | 370 | 376 | 385 | 338 |
| Total times. | 1118 | 934 | 1046 | 852 | 1032 | 873 | 783 |

That thread wound light round the bulb of the thermometer ſhould form a covering leſs warm than the ſame quantity of wool, or other raw materials of which the thread is made, ſurrounding the bulb of the thermometer in a more looſe manner, and conſequently occupying a greater ſpace, is no more than what I expected, from the idea I had formed of the cauſes of the warmth of covering; but I confeſs I was much ſurpriſed to find that there is ſo great a difference in the relative warmth of theſe two coverings, when they are employed to confine great degrees of Heat, and when the Heat they confine is much leſs in proportion to the temperature of the ſurrounding medium. This difference was very remarkable; in the Experiments with ſheep's wool, and with woollen thread, the warmth of the covering formed of 16 grains of the former, was to that formed of 16 grains of the latter, when the bulb of the thermometer was heated to 70° and cooled to 60°, as 79 to 46 (the ſurrounding medium being at 0°); but afterwards, when the thermometer had only fallen from 20° to 10° of Heat, the warmth of the wool was to that of the woollen thread only as 426 to 410; and in the Experiments with lint, and with linen thread, when the Heat was much abated, the covering of the thread appeared to be even warmer than that of the lint, though in the beginning of the Experiments, when the Heat was much greater, the lint was warmer than the thread, in the proportion of 80 to 46.

From hence it ſhould ſeem that a covering may, under certain circumſtances, be very good for confining ſmall degrees of warmth, which would be but very indifferent when made uſe of for confining a more intenſe Heat, and *vice verſa.* This, I believe, is a new fact; and, I think the knowledge of it may lead to further diſcoveries relative to the cauſes of the warmth of coverings, or the manner in which Heat makes its paſſage through them. But I forbear to enlarge upon this ſubject, till I ſhall have given an account of ſeveral other Experiments, which I think throw more light upon it, and which will conſequently render the inveſtigation eaſier and more ſatisfactory.

With a view to determine how far the power which certain bodies appear to poſſeſs of confining Heat, when made uſe of as covering, depends upon the natures of thoſe bodies, conſidered as chymical ſubſtances, or upon the chymical principles of which they are compoſed, I made the following Experiments.

As charcoal is ſuppoſed to be compoſed almoſt entirely of phlogiſton, I thought that, if that principle was the cauſe either of the conducting power, or the non-conducting power of the bodies which contain it, I ſhould diſcover it by making the Experiment with charcoal, as I had done with various other bodies. Accordingly, having filled the globe of the paſſage-thermometer with 176 grains of that ſubſtance in very fine powder, (it having been pounded in a mortar, and ſifted through a fine ſieve,) the bulb of the thermometer being ſurrounded by this powder, the inſtrument was heated in boiling water, and being afterwards plunged into a mixture of pounded ice and water, the times of cooling were obſerved as mentioned in the following Table. I afterwards repeated the Experiment with lampblack, and with very pure and very dry wood aſhes; the reſults of which Experiments were as under-mentioned:

| The bulb of the thermometer ſurrounded by | | | | |
|---|---|---|---|---|
| Heat loſt. | 176 grains of fine powder of charcoal. | 176 grains of fine powder of charcoal. | 195 grains of lampblack. | 307 grains of pure dry wood aſhes. |
| | Exp. No. 24. | Exp. No. 25. | Exp. No. 26. | Exp. No. 27. |
| 70° | — | — | — | — |
| 60° | 79″ | 91″ | 124″ | 96″ |
| 50° | 95 | 91 | 118 | 92 |
| 40° | 100 | 109 | 134 | 107 |
| 30° | 139 | 133 | 164 | 136 |
| 20° | 196 | 192 | 237 | 185 |
| 10° | 331 | 321 | 394 | 311 |
| Total times. | 940 | 937 | 1171 | 927 |

The Experiment No. 25 was ſimply a repetition of that numbered 24, and was made immediately after it; but, in moving the thermometer about in the former Experiment, the powder of charcoal which filled the globe was ſhaken a little together, and to this circumſtance I attribute the difference in the reſults of the two Experiments.

In the Experiments with lampblack and with wood aſhes, the times taken up in cooling from 70° to 60° were greater than thoſe employed in cooling from 60° to 50°; this moſt probably aroſe from the conſiderable quantity of Heat contained by theſe ſubſtances, which was firſt to be diſpoſed of, before they could receive and communicate to the ſurrounding medium that which was contained by the bulb of the thermometer.

The next Experiment I made was with *ſemen lycopodii*, commonly called witch-meal, a ſubſtance which poſſeſſes very extraordinary properties. It is almoſt impoſſible to wet it; a quantity of it ſtrewed upon the ſurface of a baſin of water, not only ſwims upon the water without being wet, but it prevents other bodies from being wet which are plunged into the water through it; ſo that a piece of money, or other ſolid body, may be taken from the bottom of the baſin by the naked hand, without wetting the hand; which is one of the tricks commonly ſhown by the jugglers in the country: this meal covers the hand, and deſcending along with it to the bottom of the baſin, defends it from the water. This ſubſtance has the appearance of an exceeding fine, light, and very moveable yellow powder, and it is very inflammable; ſo much ſo, that being blown out of a quill into the flame of a candle, it flaſhes like gunpowder, and it is made uſe of in this manner in our theatres for imitating lightning.

Conceiving that there muſt have been a ſtrong attraction between this ſubſtance and air, and ſuſpecting, from ſome circumſtances attending ſome of the foregoing Experiments, that the warmth of a covering depends not merely upon the fineneſs of the ſubſtance of which the covering is formed, and the diſpoſition of its parts, but that it ariſes in ſome meaſure from a certain attraction between the ſubſtance and the air which fills its interſtices, I thought that an Experiment with *ſemen lycopodii* might poſſibly throw ſome light upon this matter; and in this opinion I was not altogether miſtaken, as will appear by the reſults of the three following Experiments.

The bulb of the thermometer ſurrounded by 256 grs. of *ſemen lycopodii.*

| Heat loſt. | Cooled. | Cooled. | Heat acquired. | Heated. |
|---|---|---|---|---|
| | Exp. No 28. | Exp. No. 29. | | Exp. No. 30. |
| 70° | — | — | 0° | — |
| 60° | 146″ | 157″ | 10° | 230″ |
| 50° | 162 | 160 | 20° | 68 |
| 40° | 175 | 170 | 30° | 63 |
| 30° | 209 | 203 | 40° | 76 |
| 20° | 284 | 288 | 50° | 121 |
| 10° | 502 | 513 | 60° | 316 |
| — | — | — | 70° | 1585 |
| Total times. | 1478 | 1491 | — | 2459 |

In the laſt Experiment (N° 30) the reſult of which was ſo very extraordinary, the inſtrument was cooled to 0° in thawing ice, after which it was plunged ſuddenly into boiling water, where it remained till the incloſed thermometer had acquired the Heat of 70°, which took up no leſs than 2456 ſeconds, or above 40 minutes; and it had remained in the boiling water full a minute and an half before the mercury in the thermometer ſhowed the leaſt ſign of riſing. Having at length been put into motion, it roſe very rapidly 40 or 50 degrees, after which its motion gradually abating became ſo ſlow, that it took up 1585 ſeconds, or ſomething more than 26 minutes, in riſing from 60° to 70°, though the temperature of the medium in which it was placed during the whole of this time was very nearly 80°; the mercury in the barometer ſtanding but little ſhort of 27 Paris inches.

All the different ſubſtances which I had yet made uſe of in theſe Experiments for ſurrounding or covering the bulb of the thermometer, fluids excepted, had, in a greater, or in a leſs degree confined the Heat, or prevented its paſſing into or out of the thermometer ſo rapidly as it would have done, had there been nothing but air in the glaſs globe, in the centre of which the bulb of the thermometer was ſuſpended. But the great queſtion is, how, or in what manner, they produced this effect?

And firſt, it was not in conſequence of their own non-conducting powers, ſimply conſidered; for, if inſtead of being only bad conductors of Heat, we ſuppoſe them to have been totally impervious to Heat, their volumes or ſolid contents were ſo exceedingly ſmall in proportion to the capacity of the globe in which they were placed, that, had they had no effect whatever upon the air filling their interſtices, that air would have been ſufficient to have conducted all the Heat communicated, in leſs time than was actually taken up in the Experiment.

The diameter of the globe being 1,6 inches, its contents amounted to 2,14466 cubic inches; and the contents of the bulb of the thermometer being only 0,08711 of a cubic inch, (its diameter being 0,55 of an inch,) the ſpace between the bulb of the thermometer and the internal ſurface of the globe amounted to 2,14466−0,08711=2,05755 cubic inches; the whole of which ſpace was occupied by the ſubſtances by which the bulb of the thermometer was ſurrounded in the Experiments in queſtion.

But though theſe ſubſtances occupied this ſpace, they were far from *filling it;* by much the greater part of it being filled by the air which occupied the interſtices of the ſubſtances in queſtion. In the Experiment N 4, this ſpace was occupied by 16 grains of raw ſilk; and as the ſpecific gravity of raw ſilk is to that of water as 1734 to 1000, the volume of this ſilk was equal to the volume of 9,4422 grains of water; and as 1 cubic inch of water weighs 253,185 grains, its volume was equal to $\frac{9,4422}{253,185}$ =0.037294 of a cubic inch; and, as the ſpace it occupied amounted to 2,05755 cubic inches, it appears that the ſilk filled no more than about $\frac{1}{55}$ part of the ſpace in which it was confined, the reſt of that ſpace being filled with air.

In the Experiment N° 1, when the ſpace between the bulb of the thermometer and the glaſs globe, in the centre of which it was confined, was filled with nothing but air, the time taken up by the thermometer in cooling from 70° to 10° was

576 ſeconds; but in the Experiment N° 4, when this ſame ſpace was filled with 54 parts air, and 1 part raw ſilk, the time of cooling was 1284 ſeconds.

Now, ſuppoſing that the ſilk had been totally incapable of conducting any Heat at all, if we ſuppoſe, at the ſame time, that it had no power to prevent the air remaining in the globe from conducting it, in that caſe its preſence in the globe could only have prolonged the time of cooling in proportion to the quantity of the air it had diſplaced to the quantity remaining, that is to ſay, as 1 is to 54, or a little more than 10 ſeconds. But the time of cooling was actually prolonged 708 ſeconds (for in the Experiment N° 1, it was 576 ſeconds, and in the Experiment N° 4, it was 1284 ſeconds, as has juſt been obſerved); and this ſhows, that the ſilk not only did not conduct the Heat itſelf, but that it prevented the air by which its interſtices were filled from conducting it; or, at leaſt, it greatly weakened its power of conducting it.

The next queſtion which ariſes is, how air can be prevented from conducting Heat? and this neceſſarily involves another, which is, how does air conduct Heat?

If air conducted Heat, as it is probable that the metals and water, and all other ſolid bodies and unelaſtic fluids conduct it, that is to ſay, if its particles remaining in their places, the Heat paſſed from one particle to another, through the whole maſs, as there is no reaſon to ſuppoſe that the propagation of Heat is neceſſarily in right lines, I cannot conceive how the interpoſition of ſo ſmall a quantity of any ſolid body as $\frac{1}{55}$ part of the volume of the air could have effected ſo remarkable a diminution of the conducting power of the air, as appeared in the Experiment (N° 4) with raw ſilk, above mentioned.

If air and water conducted Heat in the ſame *manner*, it is more than probable that their conducting powers might be impaired by the ſame means; but when I made the Experiment with water, by filling the glaſs globe, in the centre of which the bulb of the thermometer was ſuſpended, with that fluid, and afterwards varied the Experiment, by adding 16 grains of raw ſilk to the water, I did not find that the conducting power of the water was ſenſibly impaired by the preſence of the ſilk *.

But we have juſt ſeen that the ſame ſilk, mixed with an equal volume of air, diminiſhed its conducting power in a very remarkable degree; conſequently, there is great reaſon to conclude that water and air conduct Heat in a *different manner*.

But the following Experiment, I think, puts the matter beyond all doubt.

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* The Experiment here mentioned was made in the year 1787; but the reſult of a more careful inveſtigation of the ſubject has ſince ſhown that Heat is not propagated in water in the manner here ſuppoſed. (See Eſſay VII.)

It is well known, that the power which air poſſeſſes of holding water in ſolution is augmented by Heat, and diminiſhed by cold, and that, if hot air is ſaturated with water, and if this air is afterwards cooled, a part of its water is neceſſarily depoſed.

I took a cylindrical bottle of very clear tranſparent glaſs, about 8 inches in diameter, and 12 inches high, with a ſhort and narrow neck, and ſuſpending a ſmall piece of linen rag, moderately wet, in the middle of it, I plunged it into a large veſſel of water, warmed to about 100° of Fahrenheit's thermometer, where I ſuffered it to remain till the contained air was not only warm, but thoroughly ſaturated with the moiſture which it attracted from the linen rag, the mouth of the bottle being well ſtopped up during this time with a good cork; this being done, I removed the cork for a moment, to take away the linen rag, and ſtopping up the bottle again immediately, I took it out of the warm water, and plunged it into a large cylindrical jar, about 12 inches in diameter, and 16 inches high, containing juſt ſo much ice-cold water, that, when the bottle was plunged into it, and quite covered by it, the jar was quite full.

As the jar was of very fine tranſparent glaſs, as well as the bottle, and as the cold water contained in the jar was perfectly clear, I could ſee what paſſed in the bottle moſt diſtinctly; and having taken care to place the jar upon a table near the window,

window in a very favourable light, I ſet myſelf to obſerve the appearances which ſhould take place, with all that anxious expectation which a conviction that the reſult of the Experiment muſt be deciſive, naturally inſpired.

I was certain, that the air contained in the bottle could not part with its Heat, without at the ſame time, that is to ſay, *at the ſame moment*, and *in the ſame place*, parting with a portion of its water; if, therefore, the Heat penetrated the maſs of air from the centre to the ſurface, or *paſſed through it* from particle to particle, in the ſame manner as it is probable that it paſſes through water, and all other unelaſtic fluids *, by far the greateſt part of the air contained in the bottle would part with its Heat, when *not actually in contact with the glaſs*, and a proportional part of its water being let fall at the ſame time, and in the *ſame place*, would neceſſarily deſcend in the form of rain; and, though this rain might be too fine to be viſible in its deſcent, yet I was ſure I ſhould find it at the bottom of the bottle, if not in viſible drops of water, yet in that kind of cloudy covering which cold glaſs acquires from a contact with hot ſteam or watery vapour.

But if the particles of air, inſtead of communicating their Heat from one to another, from the centre to the ſurface of the bottle, each in its turn, and for itſelf, came to the ſurface of the bottle, and

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* This opinion reſpecting the manner in which Heat is propagated in water, and other unelaſtic fluids, was afterwards found to be erroneous, as has been ſhown in the preceding Eſſay.

there depoſited its Heat and its water, I concluded that the cloudineſs occaſioned by this depoſit of water would appear all over the bottle, or, at leaſt, not more of it at the bottom than at the ſides, but rather leſs; and this I found to be the caſe in fact.

The cloudineſs firſt made its appearance upon the ſides of the bottle, near the top of it; and from thence it gradually ſpread itſelf downwards, till, growing fainter as it deſcended lower, it was hardly viſible at the diſtance of half an inch from the bottom of the bottle; and upon the bottom itſelf, which was nearly flat, there was ſcarcely the ſmalleſt appearance of cloudineſs.

Theſe appearances, I think, are eaſy to be accounted for. The air immediately in contact with the glaſs being cooled, and having depoſited a part of its water upon the ſurface of the glaſs, at the ſame time that it communicates to it its Heat, ſlides downwards by the ſides of the bottle in conſequence of its increaſed ſpecific gravity, and, taking its place at the bottom of the bottle, forces the whole maſs of hot air upwards; which, in its turn coming to the ſides of the bottle, *there* depoſits its Heat and its water, and afterwards bending its courſe downwards, this circulation is continued till all the air in the bottle has acquired the exact temperature of the water in the jar.

From hence it is clear why the firſt appearance of condenſed vapour is near the top of the bottle, as alſo why the greateſt collection of vapour is in that part, and that ſo very ſmall a quantity of it is found nearer the bottom of the bottle.

This Experiment confirmed me in an opinion which I had for ſome time entertained, that, though the particles of air individually, or each for itſelf, are capable of receiving and *tranſporting* Heat, yet air in a quieſcent ſtate, or as a fluid whoſe parts are at reſt with reſpect to each other, is not capable of conducting it, or giving it a paſſage; in ſhort, that Heat is incapable of *paſſing through a maſs of air*, penetrating from one particle of it to another, and that it is to this circumſtance that its non-conducting power is principally owing.

It is alſo to this circumſtance, in a great meaſure, that it is owing that its non-conducting power, or its apparent warmth when employed as a covering for confining Heat, is ſo remarkably increaſed upon its being mixed with a ſmall quantity of any very fine, light, ſolid ſubſtance, ſuch as the raw ſilk, fur, Eider down, &c. in the foregoing Experiments: for as I have already obſerved, though theſe ſubſtances, in the very ſmall quantities in which they were made uſe of, could hardly have prevented, in any conſiderable degree, the air from conducting, or giving a *paſſage* to the Heat, had it been capable of paſſing through it, yet they might very much impede it in the operation of tranſporting it.

But there is another circumſtance which it is neceſſary to take into the account, and that is the attraction which ſubſiſts between air and the bodies above-mentioned, and other like ſubſtances conſtituting natural and artificial clothing. For, though the incapacity of air to give a paſſage to Heat in the manner ſolid bodies permit it to paſs through them, may enable us to account for its warmth under certain circumſtances, yet the bare admiſſion of this principle does not ſeem to be ſufficient to account for the very extraordinary degrees of warmth which we find in furs and in feathers, and in various other kinds of natural and artificial clothing; nor even that which we find in ſnow; for if we ſuppoſe the particles of air to be at liberty to *carry off* the Heat which theſe bodies are meant to confine, without any other obſtruction or hindrance than that ariſing from their *vis inertiæ*, or the force neceſſary to put them in motion, it ſeems probable that the ſucceſſion of freſh particles of cold air, and the conſequent loſs of Heat, would be much more rapid than we find it to be in fact.

That an attraction, and a very ſtrong one, actually ſubſiſts between the particles of air, and the fine hair or furs of beaſts, the feathers of birds, wool, &c. appears by the obſtinacy with which theſe ſubſtances retain the air which adheres to them, even when immerſed in water, and put under the receiver of an air-pump; and that this attraction is eſſential to the warmth of theſe bodies, I think is very eaſy to be demonſtrated.

In furs, for inſtance, the attraction between the particles of air, and the fine hairs in which it is concealed, being greater than the increaſed elaſticity,

ticity, ~~or~~ repulſion of thoſe particles with regard to each other, ariſing from the Heat communicated to them by the animal body, the air in the fur, though heated, is not eaſily diſplaced; and this coat of confined air is the real barrier which defends the animal body from the external cold. This air cannot *carry off* the Heat of the animal, becauſe it is itſelf confined, by its attraction to the hair ~~or fur~~; and it tranſmits it with great difficulty, if it tranſmits at all, as has been abundantly ſhewn by the foregoing Experiments.

Hence it appears why thoſe furs which are the fineſt, longeſt, and thickeſt, are likewiſe the warmeſt; and how the furs of the beaver, of the otter, and of other like quadrupeds which live much in water, and the feathers of water-fowls, are able to confine the Heat of thoſe animals in winter, notwithſtanding the extreme coldneſs and great conducting power of the water in which they ſwim. The attraction between theſe ſubſtances, and the air which occupies their interſtices, is ſo great, that this air is not diſlodged even by the contact of water, but remaining in its place, it defends the body of the animal at the ſame time from being wet, and from being robbed of its Heat by the ſurrounding cold fluid; and it is poſſible that the preſſure of this fluid upon the covering of air confined in the interſtices of the fur, or feathers, may at the ſame time increaſe its warmth, or non-conducting power, in ſuch a manner that the animal may not, in fact, loſe more Heat when in water, than when in air: for we have ſeen by the foregoing Experiments, that, under certain circumſtances, the warmth of a covering is increaſed, by bringing its component parts nearer together, or by increaſing its denſity even at the expence of its thickneſs. But this point will be further inveſtigated hereafter.

Bears, wolves, foxes, hares, and other like quadrupeds, inhabitants of cold countries, which do not often take the water, have their fur much thicker upon their backs than upon their bellies. The heated air occupying the interſtices of the hairs of the animal tending naturally to riſe upwards, in conſequence of its increaſed elaſticity, would eſcape with much greater eaſe from the backs of quadrupeds than from their bellies, had not Providence wiſely guarded againſt this evil by increaſing the obſtructions in thoſe parts, which entangle it and confine it to the body of the animal. And this, I think, amounts almoſt to a proof of the principles aſſumed relative to the manner in which Heat is carried off by air, and the cauſes of the non-conducting power of air, or its apparent warmth, when, being combined with other bodies, it acts as a covering for confining Heat.

The ſnows which cover the ſurface of the earth in winter, in high latitudes, are doubtleſs deſigned by an all-provident Creator as a garment to defend it againſt the piercing winds from the polar regions, which prevail during the cold ſeaſon.

Theſe winds, notwithſtanding the vaſt tracts of continent over which they blow, retain their ſharpneſs as long as the ground they paſs over is covered with ſnow; and it is not till meeting with the ocean they acquire, from a contact with its waters, the Heat which the ſnows prevent their acquiring from the earth, that the edge of their coldneſs is taken off, and they gradually die away and are loſt.

The winds are always found to be much colder when the ground is covered with ſnow than when it is bare, and this extraordinary coldneſs is vulgarly ſuppoſed to be communicated to the air by the ſnow; but this is an erroneous opinion; for theſe winds are in general much colder than the ſnow itſelf.

They retain their coldneſs, becauſe the ſnow prevents them from being warmed at the expence of the earth; and this is a ſtriking proof of the uſe of the ſnows in preſerving the Heat of the earth during the winter in cold latitudes.

It is remarkable that theſe winds ſeldom blow from the poles directly towards the equator, but from the land towards the ſea. Upon the eaſtern coaſt of North America the cold winds come from the north-weſt; but upon the weſtern coaſt of Europe they blow from the north-eaſt.

That they ſhould blow towards thoſe parts where they can moſt eaſily acquire the Heat they are in ſearch of, is not extraordinary; and that they ſhould gradually ceaſe and die away, upon being warmed by a contact with the waters of the ocean, is likewiſe agreeable to the nature and cauſes of their motion; and if I might be allowed a conjecture reſpecting the principal uſe of the ſeas, or the reaſon why the proportion of water upon the ſurface of our globe is ſo great, compared to that of the land, it is to maintain a more equal temperature in the different climates, by heating or cooling the winds which at certain periods blow from the great continents.

That cold winds actually grow much milder upon paſſing over the ſea, and that hot winds are refreſhed by a contact with its waters, is very certain; and it is equally certain that the winds from the ocean are, in all climates, much more temperate than thoſe which blow from the land.

In the iſlands of Great Britain and Ireland, there is not the leaſt doubt but the great mildneſs of the climate is entirely owing to their ſeparation from the neighbouring continent by ſo large a tract of ſea; and in all ſimilar ſituations, in every part of the globe, ſimilar cauſes are found to produce ſimilar effects.

The cold north-weſt winds, which prevail upon the coaſt of North America during the winter, ſeldom extend above 100 leagues from the ſhore, and they are always found to be leſs violent, and leſs piercing, as they are further from the land.

Theſe periodical winds from the continents of Europe and North America prevail moſt towards the end of the month of February, and in the month of March; and I conceive that they contribute very eſſentially towards bringing on an early ſpring, and a fruitful ſummer, particularly when they are very violent in the month of March, and if at that time the ground is well covered with ſnow. The whole atmoſphere of the polar regions being, as it were, tranſported into the ocean by theſe winds, is there warmed and ſaturated with water: and, a great accumulation of air upon the ſea being the neceſſary conſequence of the long continuance of theſe cold winds from the ſhore, upon their ceaſing the warm breezes from the ſea neceſſarily commence, and, ſpreading themſelves upon the land far and wide, aſſiſt the returning ſun in diſmantling the earth of the remains of her winter garment, and in bringing forward into life all the manifold beauties of the new-born year.

This warmed air which comes in from the ſea, having acquired its Heat from a contact with the ocean, is, of courſe, ſaturated with water; and hence the warm ſhowers of April and May, ſo neceſſary to a fruitful ſeaſon.

The ocean may be conſidered as the great reſervoir and equalizer of Heat; and its benign influences in preſerving a proper temperature in the atmoſphere operate in all ſeaſons and in all climates.

The parching winds from the land under the torrid zone are cooled by a contact with its waters, and, in return, the breezes from the ſea, which at certain hours of the day come in to the ſhores in almoſt all hot countries, bring with them refreſhment, and, as it were, new life and vigour both to the animal and vegetable creation, fainting and melting under the exceſſive Heats of a burning ſun. What a vaſt tract of country, now the moſt fertile upon the face of the globe, would be abſolutely barren and uninhabitable on account of the exceſſive Heat, were it not for theſe refreſhing ſea-breezes! And is it not more than probable, that the extremes of heat and of cold in the different ſeaſons in the temperate and frigid zones would be quite intolerable, were it not for the influence of the ocean in preſerving an equability of temperature?

And to theſe purpoſes the ocean is wonderfully well adapted not only on account of the great power of water to abſorb Heat, and the vaſt depth and extent of the different ſeas (which are ſuch that one ſummer or one winter could hardly be ſuppoſed to have any ſenſible effect in heating or cooling this enormous maſs); but alſo on account of the continual circulation which is carried on in the ocean itſelf, by means of the currents which prevail in it. The waters under the torrid zone being carried by theſe currents towards the polar regions, are there cooled by a contact with the cold winds, and, having thus communicated their Heat to theſe inhoſpitable regions, return towards the equator, carrying with them refreſhment for thoſe parching climates.

The wiſdom and goodneſs of Providence have often been called in queſtion with regard to the diſtribution of land and water upon the ſurface of our globe, the vaſt extent of the ocean having been conſidered as a proof of the little regard that has been paid to man in this diſtribution.. But, the more light we acquire reſpecting the real conſtitution of things, and the various uſes of the different parts of the viſible creation, the leſs we ſhall be diſpoſed to indulge ourſelves in ſuch frivolous criticiſms.

END OF THE EIGHTH ESSAY.

# ESSAY IX.

AN

# EXPERIMENTAL INQUIRY

CONCERNING

# *THE SOURCE OF THE HEAT WHICH IS EXCITED BY FRICTION.*

[Read before the ROYAL SOCIETY, January 25, 1798.]

# ESSAY IX.

## An INQUIRY concerning the SOURCE of the HEAT which is EXCITED by FRICTION

[Read before the ROYAL SOCIETY, January 25, 1798.]

IT frequently happens, that in the ordinary affairs and occupations of life, opportunities preſent themſelves of contemplating ſome of the moſt curious operations of Nature; and very intereſting philoſophical experiments might often be made, almoſt without trouble or expence, by means of machinery contrived for the mere mechanical purpoſes of the arts and manufactures.

I have frequently had occaſion to make this obſervation; and am perſuaded, that a habit of keeping the eyes open to every thing that is going on in the ordinary courſe of the buſineſs of life has oftener led, as it were by accident, or in the playful excurſions of the imagination, put into action by contemplating the moſt common appearances, to uſeful doubts, and ſenſible ſchemes for inveſtigation

and

and improvement, than all the more intenſe meditations of philoſophers, in the hours expreſsly ſet apart for ſtudy.

It was by accident that I was led to make the Experiments of which I am about to give an account; and, though they are not perhaps of ſufficient importance to merit ſo formal an introduction, I cannot help flattering myſelf that they will be thought curious in ſeveral reſpects, and worthy of the honour of being made known to the Royal Society.

Being engaged, lately, in ſuperintending the boring of cannon, in the workſhops of the military arſenal at Munich, I was ſtruck with the very conſiderable degree of Heat which a braſs gun acquires, in a ſhort time, in being bored; and with the ſtill more intenſe Heat (much greater than that of boiling water, as I found by experiment) of the metallic chips ſeparated from it by the borer.

The more I meditated on theſe phænomena, the more they appeared to me to be curious and intereſting. A thorough inveſtigation of them ſeemed even to bid fair to give a farther inſight into the hidden nature of Heat; and to enable us to form ſome reaſonable conjectures reſpecting the exiſtence, or non-exiſtence, of an *igneous fluid*: a ſubject on which the opinions of philoſophers have, in all ages, been much divided.

In order that the Society may have clear and diſtinct ideas of the ſpeculations and reaſonings to which theſe appearances gave riſe in my mind, and alſo of the ſpecific objects of philoſophical inveſtigation they ſuggeſted to me, I muſt beg leave to ſtate them at ſome length, and in ſuch manner as I ſhall think beſt ſuited to anſwer this purpoſe.

From *whence comes* the Heat actually produced in the mechanical operation above mentioned?

Is it furniſhed by the metallic chips which are ſeparated by the borer from the ſolid maſs of metal?

If this were the caſe, then, according to the modern doctrines of latent Heat, and of caloric, the *capacity for Heat* of the parts of the metal, ſo reduced to chips, ought not only to be changed, but the change undergone by them ſhould be ſufficiently great to account for *all* the Heat produced.

But no ſuch change had taken place; for I found, upon taking equal quantities, by weight, of theſe chips, and of thin ſlips of the ſame block of metal ſeparated by means of a fine ſaw, and putting them, at the ſame temperature, (that of boiling water,) into equal quantities of cold water, (that is to ſay, at the temperature of 59°½ F.) the portion of water into which the chips were put was not, to all appearance, heated either leſs or more than the other portion, in which the ſlips of metal were put.

This Experiment being repeated ſeveral times, the reſults were always ſo nearly the ſame, that I could not determine whether any, or what change, had been produced in the metal, *in regard to its* *capacity for Heat*, by being reduced to chips by the borer *

From hence it is evident, that the Heat produced could not poſſibly have been furniſhed at the expence of the latent Heat of the metallic chips. But, not being willing to reſt ſatisfied with theſe trials, however concluſive they appeared to me to be, I had recourſe to the following ſtill more deciſive Experiment:

Taking a cannon, (a braſs ſix-pounder,) caſt ſolid, and rough as it came from the foundry, (ſee Fig. 1. Tab. IV.) and fixing it (horizontally) in the machine uſed for boring, and at the ſame time finiſhing the outſide of the cannon by turning, (ſee Fig. 2.) I cauſed its extremity to be cut off; and, by turning down the metal in that part, a ſolid cylinder was formed, $7\frac{3}{4}$ inches in diameter, and $9\frac{8}{10}$ inches long; which, when finiſhed, remained joined to the reſt of the metal (that which, properly ſpeaking, conſtituted the cannon) by a ſmall cylindrical neck, only $2\frac{1}{5}$ inches in diameter, and $3\frac{8}{10}$ inches long.

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* As theſe Experiments are important, it may perhaps be agreeable to the Society to be made acquainted with them in their details.

One of them was as follows:

To 4590 grains of water, at the temperature of 59°½ F. (an allowance as compenſation, reckoned in water, for the capacity for Heat of the containing cylindrical tin veſſel, being included,) were added 1016⅛ grains of gun-metal in thin ſlips, ſeparated from the gun by means of a fine ſaw, being at the temperature of 210° F. When they had remained together 1 minute, and had been well ſtirred about, by means of a ſmall rod of light wood, the Heat of the mixture was found to be = 63°.

From this Experiment, the *ſpecific Heat* of the metal, calculated according to the rule given by Dr. CRAWFORD, turns out to be = 0.1100, that of water being = 1.0000.

An Experiment was afterwards made with the metallic chips, as follows:

To the ſame quantity of water as was uſed in the Experiment above mentioned, at the ſame temperature, (*viz.* 59°½,) and in the ſame cylindrical tin veſſel, were now put 1016⅛ grains of metallic chips of gun-metal, bored out of the ſame gun from which the ſlips uſed in the foregoing Experiment were taken, and at the ſame temperature (210°). The Heat of the mixture, at the end of 1 minute, was juſt 63°, as before; conſequently the ſpecific Heat of theſe metallic chips was = 0.1100. Each of the above Experiments was repeated three times, and always with nearly the ſame reſults.

This ſhort cylinder, which was ſupported in its horizontal poſition, and turned round its axis, by means of the neck by which it remained united to the cannon, was now bored with the horizontal borer uſed in boring cannon; but its bore, which was 3.7 inches in diameter, inſtead of being continued through its whole length (9.8 inches) was only 7.2 inches in length; ſo that a ſolid bottom was left to this hollow cylinder, which bottom was 2.6 inches in thickneſs.

This cavity is repreſented by dotted lines in Fig. 2; as alſo in Fig. 3. where the cylinder is repreſented on an enlarged ſcale.

This cylinder being deſigned for the expreſs purpoſe of generating Heat *by friction*, by having a blunt borer forced againſt its ſolid bottom at the ſame time that it ſhould be turned round its axis by the force of horſes, in order that the Heat accumulated in the cylinder might from time to time be meaſured, a ſmall round hole, (ſee *d*, *e*, Fig. 3.) 0.37 of an inch only in diameter, and 4.2 inches in

depth,

depth, for the purpoſe of introducing a ſmall cylindrical mercurial thermometer, was made in it, on one ſide, in a direction perpendicular to the axis of the cylinder, and ending in the middle of the ſolid part of the metal which formed the bottom of its bore.

The ſolid contents of this hollow cylinder, excluſive of the cylindrical neck by which it remained united to the cannon, were 385¼ cubic inches, Engliſh meaſure; and it weighed 113.13lb. Avoirdupois: as I found, on weighing it at the end of the courſe of Experiments made with it, and after it had been ſeparated from the cannon with which, during the Experiments, it remained connected*.

### *Experiment*, N° 1.

This Experiment was made in order to aſcertain how much Heat was actually generated by friction, when a blunt ſteel borer being ſo forcibly ſhoved (by means of a ſtrong ſcrew) againſt the bottom of the bore of the cylinder, that the preſſure againſt it was equal to the weight of about 10000 lb. Avoirdupois, the cylinder was turned round on its axis (by the force of horſes) at the rate of about 32 times in a minute.

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* For fear I ſhould be ſuſpected of prodigality in the proſecution of my philoſophical reſearches, I think it neceſſary to inform the Society, that the cannon I made uſe of in this Experiment was not ſacrificed to it. The ſhort hollow cylinder which was formed at the end of it, was turned out of a cylindrical maſs of metal, about 2 feet in length, projecting beyond the muzzle of the gun, called in the German language the *verlorner kopf*, (the head of the cannon to be thrown away,) and which is repreſented in Fig. 1.

This original projection, which is cut off before the gun is bored, is always caſt with it, in order that, by means of the preſſure of its weight on the metal in the lower part of the mould, during the time it is cooling, the gun may be the more compact in the neighbourhood of the muzzle; where, without this precaution, the metal would be apt to be porous, or full of honeycombs.

This machinery, as it was put together for the Experiment, is repreſented by Fig. 2. W is a ſtrong horizontal iron bar, connected with proper machinery carried round by horſes, by means of which the cannon was made to turn round it axis.

To prevent, as far as poſſible, the loſs of any part of the Heat that was generated in the Experiment, the cylinder was well covered up with a fit coating of thick and warm flannel, which was carefully wrapped round it, and defended it on every ſide from the cold air of the atmoſphere. This covering is not repreſented in the drawing of the apparatus, Fig. 2.

I ought to mention, that the borer was a flat piece of hardened ſteel, 0.63 of an inch thick, 4 inches long, and nearly as wide as the cavity of the bore of the cylinder, namely, 3½ inches. Its corners were rounded off at its end, ſo as to make it fit the hollow bottom of the bore; and it was firmly faſtened to the iron bar (*m*) which kept it in its place. The area of the ſurface by which its end was in contact with the bottom of the bore of the cylinder was nearly 2⅓ inches. This borer, which is diſtinguiſhed by the letter *n*, is repreſented in moſt of the figures.

At the beginning of the Experiment, the temperature of the air in the ſhade, as alſo that of the cylinder, was juſt 60° F.

At the end of 30 minutes, when the cylinder had made 960 revolutions about its axis, the horſes being ſtopped, a cylindrical mercurial thermometer, whoſe bulb was $\frac{32}{100}$ of an inch in diameter, and $3\frac{1}{4}$ inches in length, was introduced into the hole made to receive it, in the ſide of the cylinder, when the mercury roſe almoſt inſtantly to 130°.

Though the Heat could not be ſuppoſed to be quite equally diſtributed in every part of the cylinder, yet, as the length of the bulb of the thermometer was ſuch that it extended from the axis of the cylinder to near its ſurface, the Heat indicated by it could not be very different from that of the *mean temperature* of the cylinder; and it was on this account that a thermometer of that particular form was choſen for this Experiment.

To ſee how faſt the Heat eſcaped out of the cylinder, (in order to be able to make a probable conjecture reſpecting the quantity given off by it, during the time the Heat generated by the friction was accumulating,) the machinery ſtanding ſtill, I ſuffered the thermometer to remain in its place near three quarters of an hour, obſerving and noting down, at ſmall intervals of time, the height of the temperature indicated by it.

Thus,

| Thus, at the end of | The Heat, as ſhown by the thermometer, was |
|---|---|
| 4 minutes - - - - - - | 126° |
| after 5 minutes, always reckoning from the firſt obſervation, - - - - | 125° |
| at the end of 7 minutes - - - - - - | 123° |
| 12 ——— - - - - - - | 120° |
| 14 ——— - - - - - - | 119° |
| 16 ——— - - - - - - | 118° |
| 20 ——— - - - - - - | 116° |
| 24 ——— - - - - - - | 115° |
| 28 ——— - - - - - - | 114° |
| 31 ——— - - - - - - | 113° |
| 34 ——— - - - - - - | 112° |
| 37½ ——— - - - - - - | 111° |
| and when 41 minutes had elapſed - | 110° |

Having taken away the borer, I now removed the metallic duſt, or rather ſcaly matter, which had been detached from the bottom of the cylinder by the blunt ſteel borer, in this Experiment; and, having carefully weighed it, I found its weight to be 837 grains Troy.

Is it poſſible that the very conſiderable quantity of Heat that was produced in this Experiment (a quantity which actually raiſed the temperature of above 113lb. of gun-metal at leaſt 70 degrees of FAHRENHEIT's thermometer, and which, of courſe, would have been capable of melting 6½ lb. of ice, or of cauſing near 5lb. of ice-cold water to boil) could have been furniſhed by ſo inconſiderable a quantity of metallic duſt? and this merely in conſequence of *a change* of its capacity for Heat?

As the weight of this duſt (837 grains Troy) amounted to no more than $\frac{1}{948}$th part of that of the cylinder, it muſt have loſt no leſs than 948 degrees of Heat, to have been able to have raiſed the temperature of the cylinder 1 degree; and conſequently it muſt have given off 66360 degrees of Heat, to have produced the effects which were actually found to have been produced in the Experiment!

But, without inſiſting on the improbability of this ſuppoſition, we have only to recollect, that from the reſults of actual and deciſive Experiments, made for the expreſs purpoſe of aſcertaining that fact, the capacity for Heat, of the metal of which great guns are caſt, *is not ſenſibly changed* by being reduced to the form of metallic chips, in the operation of boring cannon; and there does not ſeem to be any reaſon to think that it can be much changed, if it be changed at all, in being reduced to much ſmaller pieces, by means of a borer that is leſs ſharp.

If the Heat, or any conſiderable part of it, were produced in conſequence of a change in the capacity for Heat of a part of the metal of the cylinder, as ſuch change could only be *ſuperficial*, the cylinder would by degrees be *exhauſted;* or the quantities of Heat produced, in any given ſhort ſpace of time, would be found to diminiſh gradually, in ſucceſſive Experiments. To find out if this really happened or not, I repeated the laſt-mentioned Experiment ſeveral times, with the utmoſt care; but I did not diſcover the ſmalleſt ſign of exhauſtion in the metal, notwithſtanding the large quantities of Heat actually given off.

Finding ſo much reaſon to conclude, that the Heat generated in theſe Experiments, or *excited*, as I would rather chooſe to expreſs it, was not furniſhed *at the expence of the latent Heat* or *combined caloric* of the metal, I puſhed my inquiries a ſtep farther, and endeavoured to find out whether the air did, or did not, contribute any thing in the generation of it.

### *Experiment*, N° 2.

As the bore of the cylinder was cylindrical, and as the iron bar (*m*), to the end of which the blunt ſteel borer was fixed, was ſquare, the air had free acceſs to the inſide of the bore, and even to the bottom of it, where the friction took place by which the Heat was excited.

As neither the metallic chips produced in the ordinary courſe of the operation of boring braſs cannon, nor the finer ſcaly particles produced in the laſt-mentioned Experiments by the friction of the blunt borer, ſhowed any ſigns of calcination, I did not ſee how the air could poſſibly have been the cauſe of the Heat that was produced; but, in an inveſtigation of this kind, I thought that no pains ſhould be ſpared to clear away the rubbiſh, and leave the ſubject as naked and open to inſpection as poſſible.

In order, by one deciſive Experiment, to determine whether the air of the atmoſphere had any part, or not, in the generation of the Heat, I contrived to repeat the Experiment, under circumſtances in which *it was evidently impoſſible for it to produce any effect whatever.* By means of a piſton exactly fitted to the mouth of the bore of the cylinder, through the middle of which piſton the ſquare iron bar, to the end of which the blunt ſteel borer was fixed, paſſed in a ſquare hole made perfectly air-tight, the acceſs of the external air, to the inſide of the bore of the cylinder, was effectually prevented. (In Fig. 3. this piſton (*p*) is ſeen in its place; it is likewiſe ſhown in Fig. 7 and 8.).

I did not find, however, by this Experiment, that the excluſion of the air diminiſhed, in the ſmalleſt degree, the quantity of Heat excited by the friction.

There ſtill remained one doubt, which, though it appeared to me to be ſo ſlight as hardly to deſerve any attention, I was however deſirous to remove. The piſton which cloſed the mouth of the bore of the cylinder, in order that it might be air-tight, was fitted into it with ſo much nicety, by means of its collars of leather, and preſſed againſt it with ſo much force, that, notwithſtanding its being oiled, it occaſioned a conſiderable degree of friction, when the hollow cylinder was turned round its axis. Was not the Heat produced, or at leaſt ſome part of it, occaſioned by this friction of the piſton? and, as the external air had free acceſs to the extremity of the bore, where it came in contact with the piſton, is it not poſſible that this air may have had ſome ſhare in the generation of the Heat produced?

## *Experiment*, N° 3.

A quadrangular oblong deal box, (ſee Fig. 4.) water-tight, 11$\frac{1}{2}$ Engliſh inches long, 9$\frac{4}{10}$ inches wide, and 9$\frac{6}{10}$ inches deep, (meaſured in the clear,) being provided, with holes or ſlits in the middle of each of its ends, juſt large enough to receive, the one, the ſquare iron rod to the end of which the blunt ſteel borer was faſtened, the other, the ſmall cylindrical neck which joined the hollow cylinder to the cannon; when this box (which was occaſionally cloſed above, by a wooden cover or lid moving on hinges) was put into its place; that is to ſay, when, by means of the two vertical openings or ſlits in its two ends, (the upper parts of which openings were occaſionally cloſed, by means of narrow pieces of wood ſliding in vertical grooves,) the box (*g*, *h*, *i*, *k*, Fig. 3.) was fixed to the machinery, in ſuch a manner that its bottom (*i*, *k*,) being in the plane of the horizon, its axis coincided with the axis of the hollow metallic cylinder; it is evident, from the deſcription, that the hollow metallic cylinder would occupy the middle of the box, without touching it on either ſide (as it is repreſented in Fig. 3.); and that, on pouring water into the box, and filling it to the brim, the cylinder would be completely covered, and ſurrounded on every ſide, by that fluid. And farther, as the box was held faſt by the ſtrong ſquare iron rod (*m*), which paſſed, in a *ſquare hole*, in the centre of one of its ends, (*a*, Fig. 4.) while the round or cylindrical neck, which joined the hollow cylinder to the end of the cannon, could turn round freely on its axis in the *round hole* in the centre of the other end of it, it is evident that the machinery could be put in motion, without the leaſt danger of forcing the box out of its place, throwing the water out of it, or deranging any part of the apparatus.

Every thing being ready, I proceeded to make the Experiment I had projected, in the following manner:

The hollow cylinder having been previouſly cleaned out, and the inſide of its bore wiped with a clean towel till it was quite dry, the ſquare iron bar, with the blunt ſteel borer fixed to the end of it, was put into its place; the mouth of the bore of the cylinder being cloſed at the ſame time, by means of the circular piſton, through the centre of which the iron bar paſſed.

This being done, the box was put in its place, and the joinings of the iron rod, and of the neck of the cylinder, with the two ends of the box, having been made water-tight, by means of collars of oiled leather, the box was filled with cold water, (*viz.* at the temperature of 60°,) and the machine was put in motion.

The reſult of this beautiful Experiment was very ſtriking, and the pleaſure it afforded me amply repaid me for all the trouble I had had, in contriving and arranging the complicated machinery uſed in making it.

The cylinder, revolving at the rate of about 32 times in a minute, had been in motion but a ſhort time, when I perceived, by putting my hand into the water, and touching the outſide of the cylinder, that Heat was generated; and it was not long before the water which ſurrounded the cylinder began to be ſenſibly warm.

At the end of 1 hour I found, by plunging a thermometer into the water in the box, (the quantity of which fluid amounted to 18.77 lb. Avoirdupois, or 2¼ wine gallons,) that its temperature had been raiſed no leſs than 47 degrees; being now 107° of FAHRENHEIT's ſcale.

When 30 minutes more had elapſed, or 1 hour and 30 minutes after the machinery had been put in motion, the Heat of the water in the box was 142°.

At the end of 2 hours, reckoning from the beginning of the Experiment, the temperature of the water was found to be raiſed to 178°.

At 2 hours 20 minutes it was at 200°; and at 2 hours 30 minutes it ACTUALLY BOILED!

It would be difficult to deſcribe the ſurpriſe and aſtoniſhment expreſſed in the countenances of the

the by-ſtanders, on ſeeing ſo large a quantity of cold water heated, and actually made to boil, without any fire.

Though there was, in fact, nothing that could juſtly be conſidered as ſurpriſing in this event, yet I acknowledge fairly that it afforded me a degree of childiſh pleaſure, which, were I ambitious of the reputation of a *grave philoſopher*, I ought moſt certainly rather to hide than to diſcover.

The quantity of Heat excited and accumulated in this Experiment was very conſiderable; for, not only the water in the box, but *alſo* the box itſelf, (which weighed 15¼ lb.) and the hollow metallic cylinder, and that part of the iron bar which, being ſituated within the cavity of the box, was immerſed in the water, were heated 150 degrees of FAHRENHEIT'S ſcale; *viz.* from 60° (which was the temperature of the water, and of the machinery, at the beginning of the Experiment) to 210°, the Heat of boiling water at Munich.

The total quantity of Heat generated may be eſtimated with ſome conſiderable degree of preciſion, as follows:

| Of the Heat excited there appears to have been actually accumulated, | Quantity of ice-cold water which, with the given quantity of Heat, might have been heated 180 degrees, or made to boil. In Avoirdupois weight. lb. |
|---|---|
| In the water contained in the wooden box, 18¾ lb. Avoirdupois, heated 150 degrees, namely, from 60° to 210° F. | 15.2 |
| In 113.13lb. of gun-metal, (the hollow cylinder,) heated 150 degrees; and, as the capacity for Heat of this metal is to that of water as 0.1100 to 1.0000, this quantity of Heat would have heated 12½ lb. of water the ſame number of degrees | 10.37 |
| In 36.75 cubic inches of iron, (being that part of the iron bar to which the borer was fixed which entered the box,) heated 150 degrees; which may be reckoned equal in capacity for Heat to 1.21lb. of water | 1.01 |
| *N. B.* No eſtimate is here made of the Heat accumulated in the wooden box, nor of that diſperſed during the Experiment. | |
| Total quantity of ice-cold water which, with the Heat actually generated by friction, and accumulated in 2 hours and 30 minutes, might have been heated 180 degrees, or made to boil | 26.58 |

From the knowledge of the *quantity* of Heat actually produced in the foregoing Experiment, and of the *time* in which it was generated, we are enabled to aſcertain *the velocity of its production*, and to determine how large a fire muſt have been, or how much fuel muſt have been conſumed, in order that, in burning equably, it ſhould have produced by combuſtion the ſame quantity of Heat in the ſame time.

In one of Dr. CRAWFORD's Experiments, (ſee his Treatiſe on Heat, p. 321,) 37 lb. 7 oz. Troy, = 181920 grains, of water, were heated $2\frac{1}{10}$ degrees of FAHRENHEIT's thermometer, with the Heat generated in the combuſtion of 26 grains of wax. This gives 382032 grains of water heated 1 degree with 26 grains of wax; or $14693\frac{14}{26}$ grains of water heated 1 degree, or $\frac{14693}{180} = 81.631$ grains heated 180 degrees, with the Heat generated in the combuſtion of 1 grain of wax.

The quantity of ice-cold water which might have been heated 180 degrees, with the Heat generated by friction in the before-mentioned Experiment, was found to be 26.58 lb. Avoirdupois, = 188060 grains; and, as 81.631 grains of ice-cold water require the Heat generated in the combuſtion of 1 grain of wax, to heat it 180 degrees, the former quantity of ice-cold water, namely 188060 grains, would require the combuſtion of no leſs than 2303.8 grains ($= 4\frac{8}{10}$ oz. Troy) of wax, to heat it 180 degrees.

As the Experiment (N° 3) in which the given quantity of Heat was generated by friction, laſted 2 hours and 30 minutes, = 150 minutes, it is neceſſary, for the purpoſe of aſcertaining how many wax-candles of any given ſize muſt burn together, in order that in the combuſtion of them the given quantity of Heat may be generated in the given time, and conſequently *with the ſame celerity* as that with which the Heat was generated by friction in the Experiment, that the ſize of the candles ſhould be determined, and the quantity of wax conſumed in a given time by each candle, in burning equably, ſhould be known.

Now I found by an Experiment, made on purpoſe to finiſh theſe computations, that when a good wax-candle, of a moderate ſize, $\frac{3}{4}$ of an inch in diameter, burns with a clear flame, juſt 49 grains of wax are conſumed in 30 minutes. Hence it appears, that 245 grains of wax would be conſumed by ſuch a candle in 150 minutes; and that, to burn the quantity of wax (= 2303.8 grains) neceſſary to produce the quantity of Heat actually obtained by friction in the Experiment in queſtion, and in the given time, (150 minutes,) *nine candles*, burning at once, would not be ſufficient; for 9 multiplied into 245 (the number of grains conſumed by each candle in 150 minutes) amounts to no more than 2205 grains; whereas the quantity of wax neceſſary to be burnt, in order to produce the given quantity of Heat, was found to be 2303.8 grains.

From the reſult of theſe computations it appears, that the quantity of Heat produced equably, or in a continual ſtream, (if I may uſe that expreſſion,) by the friction of the blunt ſteel borer againſt the bottom of the hollow metallic cylinder, in the Experiment under conſideration, was *greater* than that produced equably in the combuſtion of *nine wax-candles*, each ¾ of an inch in diameter, all burning together, or at the ſame time, with clear bright flames.

As the machinery uſed in this Experiment could eaſily be carried round by the force of one horſe, (though, to render the work lighter, two horſes were actually employed in doing it,) theſe computations ſhow further how large a quantity of Heat might be produced, by proper mechanical contrivance, merely by the ſtrength of a horſe, without either fire, light, combuſtion, or chemical decompoſition ; and, in a caſe of neceſſity, the Heat thus produced might be uſed in cooking victuals.

But no circumſtances can be imagined, in which this method of procuring Heat would not be diſadvantageous ; for, more Heat might be obtained by uſing the fodder neceſſary for the ſupport of a horſe, as fuel.

As ſoon as the laſt-mentioned Experiment (N° 3.) was finiſhed, the water in the wooden box was let off, and the box removed ; and the borer being taken out of the cylinder, the ſcaly metallic powder, which had been produced by the friction of the borer againſt the bottom of the

the cylinder, was collected, and, being carefully weighed, was found to weigh 4145 grains, or about $8\frac{2}{3}$ oz. Troy.

As this quantity was produced in $2\frac{1}{2}$ hours, this gives 824 grains for the quantity produced *in half an hour.*

In the firſt Experiment, which laſted only *half an hour*, the quantity produced was 837 grains.

In the Experiment N° 1. the quantity of Heat generated, in *half an hour*, was found to be equal to that which would be required to heat 5 lb. Avoirdupois of ice-cold water 180 degrees, or cauſe it to boil.

According to the reſult of the Experiment N° 3. the Heat generated in *half an hour* would have cauſed 5.31 lb. of ice-cold water to boil. But, in this laſt-mentioned Experiment, the Heat generated being more effectually confined, leſs of it was loſt; which accounts for the difference of the reſults of the two Experiments.

It remains for me to give an account of one Experiment more, which was made with this apparatus. I found by the Experiment N° 1. how much Heat was generated when the air had free acceſs to the metallic ſurfaces which were rubbed together. By the Experiment N° 2. I found that the quantity of Heat generated was not ſenſibly diminiſhed when the free acceſs of the air was prevented; and by the reſult of N° 3. it appeared that the generation of the Heat was not prevented, or retarded, by keeping the apparatus immerſed in water. But as, in this laſt-mentioned Experiment, the water, though it ſurrounded the hollow metallic cylinder on every ſide, externally, was not ſuffered to enter the cavity of its bore, (being prevented by the piſton,) and conſequently did not come into contact with the metallic ſurfaces where the Heat was generated; to ſee what effects would be produced by giving the water free acceſs to theſe ſurfaces, I now made the

## *Experiment*, N° 4.

The piſton which cloſed the end of the bore of the cylinder being removed, the blunt borer and the cylinder were once more put together; and the box being fixed in its place, and filled with water, the machinery was again put in motion.

There was nothing in the reſult of this Experiment that renders it neceſſary for me to be very particular in my account of it. Heat was generated, as in the former Experiments, and, to all appearance, quite as rapidly; and I have no doubt but the water in the box would have been brought to boil, had the Experiment been continued as long as the laſt. The only circumſtance that ſurpriſed me was, to find how little difference was occaſioned in the noiſe made by the borer in rubbing againſt the bottom of the bore of the cylinder, by filling the bore with water. This noiſe, which was very grating to the ear, and ſometimes almoſt inſupportable, was, as nearly as I could judge of it, quite as loud, and as diſagreeable, when the ſurfaces rubbed together were wet with water, as when they were in contact with air.

By meditating on the reſults of all theſe Experiments, we are naturally brought to that great queſtion which has ſo often been the ſubject of ſpeculation among philoſophers; namely,

What is Heat?—Is there any ſuch thing as an *igneous fluid?*—Is there any thing that can with propriety be called *caloric?*

We have ſeen that a very conſiderable quantity of Heat may be excited in the Friction of two metallic ſurfaces, and given off in a conſtant ſtream or flux, *in all directions*, without interruption or intermiſſion, and without any ſigns of diminution or exhauſtion.

From whence came the Heat which was continually given off in this manner, in the foregoing Experiments? Was it furniſhed by the ſmall particles of metal, detached from the larger ſolid maſſes, on their being rubbed together? This, as we have already ſeen, could not poſſibly have been the caſe.

Was it furniſhed by the air? This could not have been the caſe; for, in three of the Experiments, the machinery being kept immerſed in water, the acceſs of the air of the atmoſphere was completely prevented.

Was it furniſhed by the water which ſurrounded the machinery? That this could not have been the caſe is evident: *firſt*, becauſe this water was continually *receiving Heat* from the machinery, and could not, at the ſame time, be *giving to*, and *receiving Heat from*, the ſame body; and *ſecondly*, becauſe there was no chemical decompoſition of any part of this water. Had any ſuch decompoſition taken place, (which indeed could not reaſonably have been expected,) one of its component elaſtic fluids (moſt probably inflammable air) muſt, at the ſame time, have been ſet at liberty, and, in making its eſcape into the atmoſphere, would have been detected; but though I frequently examined the water to ſee if any air bubbles roſe up through it, and had even made preparations for catching them, in order to examine them, if any ſhould appear, I could perceive none; nor was there any ſign of decompoſition of any kind whatever, or other chemical proceſs, going on in the water.

Is it poſſible that the Heat could have been ſupplied by means of the iron bar to the end of which the blunt ſteel borer was fixed? or by the ſmall neck of gun-metal by which the hollow cylinder was united to the cannon? Theſe ſuppoſitions appear more improbable even than either of thoſe before mentioned; for Heat was continually going off, or *out of the machinery*, by both theſe paſſages, during the whole time the Experiment laſted.

And, in reaſoning on this ſubject, we muſt not forget to conſider that moſt remarkable circumſtance, that the ſource of the Heat generated by friction, in theſe Experiments, appeared evidently to be *inexhauſtible*.

It is hardly neceſſary to add, that any thing which any *inſulated* body, or ſyſtem of bodies, can continue to furniſh *without limitation*, cannot poſſibly be *a material ſubſtance:* and it appears to me to be extremely difficult, if not quite impoſſible, to form any diſtinct idea of any thing, capable of being excited and communicated, in the manner the Heat was excited and communicated in theſe Experiments, except it be MOTION.

I am very far from pretending to know how, or by what means, or mechanical contrivance, that particular kind of motion in bodies, which has been ſuppoſed to conſtitute Heat, is excited, continued, and propagated, and I ſhall not preſume to trouble the Society with mere conjectures; particularly on a ſubject which, during ſo many thouſand years, the moſt enlightened philoſophers have endeavoured, but in vain, to comprehend.

But, although the mechaniſm of Heat ſhould, in fact, be one of thoſe myſteries of nature which are beyond the reach of human intelligence, this ought by no means to diſcourage us, or even leſſen our ardour, in our attempts to inveſtigate the laws of its operations. How far can we advance in any of the paths which ſcience has opened to us, before we find ourſelves enveloped in thoſe thick miſts which, on every ſide, bound the horizon of the human intellect? But how ample, and how intereſting, is the field that is given us to explore!

Nobody, ſurely, in his ſober ſenſes, has ever pretended to underſtand the mechaniſm of gravitation; and yet what ſublime diſcoveries was our immortal NEWTON enabled to make, merely by the inveſtigation of the laws of its action!

The effects produced in the world by the agency of Heat are probably *juſt as extenſive*, and quite as important, as thoſe which are owing to the tendency of the particles of matter towards each other; and there is no doubt but its operations are, in all caſes, determined by laws equally immutable.

Before I finiſh this Eſſay, I would beg leave to obſerve, that although, in treating the ſubject I have endeavoured to inveſtigate, I have made no mention of the names of thoſe who have gone over the ſame ground before me, nor of the ſucceſs of their labours; this omiſſion has not been owing to any want of reſpect for my predeceſſors, but was merely to avoid prolixity, and to be more at liberty to purſue, without interruption, the natural train of my own ideas.

## DESCRIPTION *of the* FIGURES.

Fig. 1 ſhows the cannon uſed in the foregoing Experiments, in the ſtate it was in when it came from the foundry.

Fig. 2 ſhows the machinery uſed in the Experiments N° 1 and N° 2. The cannon is ſeen fixed in the machine uſed for boring cannon. W is a ſtrong iron bar, (which, to ſave room in the drawing, is repreſented as broken off,) which bar, being united with machinery (not expreſſed in the figure) that is carried round by horſes, cauſes the cannon to turn round its axis.

*m* is a ſtrong iron bar, to the end of which the blunt borer is fixed; which, by being forced againſt the bottom of the bore of the ſhort hollow cylinder that remains connected by a ſmall cylindrical neck to the end of the cannon, is uſed in generating Heat by friction.

Fig. 3 ſhows, on an enlarged ſcale, the ſame hollow cylinder that is repreſented on a ſmaller ſcale in the foregoing Figure. It is here ſeen connected with the wooden box (*g*, *h*, *i*, *k*,) uſed in the Experiments N° 3 and N° 4. when this hollow cylinder was immerſed in water.

*p*, which is marked by dotted lines, is the piſton which cloſed the end of the bore of the cylinder.

*n* is the blunt borer ſeen ſidewiſe.

*d*, *e*, is the ſmall hole by which the thermometer was introduced, that was uſed for aſcertaining the Heat of the cylinder. To ſave room in the drawing, the cannon is repreſented broken off near its muzzle; and the iron bar, to which the blunt borer is fixed, is repreſented broken off at *m*.

Fig. 4, is a perſpective view of the wooden box, a ſection of which is ſeen in the foregoing Figure. (See *g*, *h*, *i*, *k*, Fig. 3.)

Fig. 5 and 6, repreſent the blunt borer *n*, joined to the iron bar *m*, to which it was faſtened.

Fig. 7 and 8, repreſent the ſame borer, with its iron bar, together with the piſton which, in the Experiments N° 2 and N° 3. was uſed to cloſe the mouth of the hollow cylinder.

END OF THE SECOND VOLUME.

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পুস্তক কালেজ ফোর্টউলিয়ম

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